Disequilibrium economics: some comments about its nature, origins and fate. A review essay of "Transforming Modern Macroeconomics, The Relationship of Micro and Macroeconomics in Historical Perspective" (2013)

Goulven Rubin

Disequilibrium economics: some comments about its nature, origins and fate

A review essay of *Transforming Modern Macroeconomics, The Relationship of Micro and Macroeconomics in Historical Perspective* (2013) by Roger Backhouse and Mauro Boianovsky

Goulven Rubin[1]

Introduction

In 2013, Roger Backhouse and Mauro Boianovsky published *Transforming Modern Macroeconomics Exploring Disequilibrium Microfoundations, 1956-2003*. This book is the first comprehensive history of the "search for disequilibrium microfoundations" or disequilibrium theories. It is more than an impressive survey though. According to Backhouse and Boianovsky, when they recount the history of their field, practitioners, like Blanchard (2000) or Woodford (1999), tend to indulge in myth making. The standard story explains how, in the 1970s, macroeconomics was overturned by the advent of the search for microfoundations narrowly conceived as the elaboration of miniature general equilibrium models in which representative agents are optimizing, all markets clear and there is no room for involuntary unemployment. As a matter of fact, this presentation leaves a gaping hole. It ignores the existence of a search for *disequilibrium* microfoundations, "well under way long before the 1970s" (2013: 2), and resulting in a large body of works around the 1970s. Backhouse and Boianovsky propose to show us how repairing this omission changes our understanding of macroeconomics' recent history. While being sympathetic to this general undertaking, I will point two weaknesses of the book. Firstly, their presentation of the origins of the search for disequilibrium microfoundations downplays the interactions between key players like Patinkin, Clower, Arrow or Hahn. As a consequence, they tend to overemphasize the heterogeneity of their approaches. Secondly, they argue that disequilibrium theories left a deep mark on contemporary macroeconomics. But this claim is not supported by a step by step analysis. Reading their book will also be the occasion to take stock and identify the issues that remain unanswered and need to be clarified in order to complete the history of disequilibrium macroeconomics.

1. An outline of *Transforming Modern Macroeconomics*

[1] EQUIPPE and University of Lille 2 Law and Health, goulven.rubin@univ-lille2.fr.

In their introduction, Backhouse and Boianovsky adopt a very encompassing definition of disequilibrium. According to them, disequilibrium occurs when the state of rest that is reached on the market goes along with agents that are off the "conventionally defined supply and demand schedules" (page 9). They add that this outcome may arise when "competition is imperfect, or monopolistic" and when "agents have mistaken expectations or asymmetric information, or are bound by long-term or even implicit contracts".

Starting from this definition, Backhouse and Boianovsky's history of the search for disequilibrium microfoundations falls into three parts. Chapters 3 to 5 focus on the history of disequilibrium macroeconomics or aggregative models. Chapter 7 identifies a separate source of disequilibrium economics in the works of microeconomists like Arrow, Hahn and Negishi. Finally, chapters 8 and 9 discuss the fate of the whole current of thought from the 1970s to Woodford (2003). Chapters 2 and 6 are asides to the main thread of the book. The former offers a condensed history of macroeconomics from Keynes (1936) to Woodford (2003) whereas the latter presents the birth and the evolution of Lucas' research program or "equilibrium microfoundations".

Chapter 3 on "Don Patinkin and the Neoclassical Synthesis" is the real opening chapter on the history of disequilibrium theories. Backhouse and Boianovsky relate Patinkin's works to the ideas of Hicks (1939) and to the works of Lange (1942, 1945), showing that disequilibrium economics dates from the late 1930s. Then they present the content of chapter 13 of *Money, Interest and Prices* (1956) but end up being quite critical with respect to Patinkin. According to them he failed to "realize the full significance of what he was doing" (page 41).

Chapter 4 presents the contributions of Clower and Leijonhufvud. Like d'Autume (2000) or Benassy (1984), Backhouse and Boianovsky consider that Clower (1965) was the key contribution. The concept of "dual decision hypothesis" was "crucial to virtually all the ensuing literature" (page 45) showing more clearly than Patinkin had done how trading out of equilibrium cause supply and demand functions to be different. An important contribution of this chapter is the discussion of unpublished manuscripts coming from Duke University's archives that shed light on the path leading to Clower's 1965's contribution. Though these archives have been available for a long time, it is the first work that attempts to use them in a comprehensive manner. Drawing on this material, the authors conclude that Clower developed his insights by following an independent research program centered on the dynamics of stock-flow models. Finally they present the contribution of Leijonhufvud and his collaboration with Clower in the 1970s. Clower and Leijonhufvud were both dissatisfied by the representation of the market offered by Walrasian theory and were looking for something more realistic. They had in mind a theory without any price rigidities and their focus was on market process and dynamics.

The core of chapter 5 surveys the main macro models inspired by Patinkin, Clower and Leijonhufvud's contributions. After a presentation of Solow and Stiglitz (1968), the authors discuss the models of Barro and Grossman (1971) and the PhD thesis of Bénassy (1973). Then they explain how these models were refined and applied to a variety of issues like consumption behaviors, international trade or planed economies. Finally, they argue that a number of contributions in the 1970s were developed to explain why prices could remain rigid. They mention the works of Stiglitz on asymmetric information, the models of staggered prices and wages developed by Phelps, Taylor or Fisher and the literature on implicit contracts.

Chapter 7 argues that internal criticism of the Walrasian model in its Arrow-Debreu version was the second source of disequilibrium theories. Arrow's article "Towards a theory of price adjustment" (1959) was the starting point of this literature. Arrow pointed out the lack of integration of microeconomics and macroeconomics. To bring microeconomics closer to reality, it was necessary to drop the auctioneer, to allow disequilibrium trade and assume that agents were price makers. From this article and the resulting contributions of Negishi and Hahn, Backhouse and Boianovsky conclude that disequilibrium and imperfect competition are intimately related. This "general equilibrium" approach of disequilibrium is also found in the works of Drèze, Younès, Bénassy, Grandmont and Laroque.

Chapter 8 assesses the importance taken by disequilibrium theories during the 1970s. Backhouse and Boianovsky show that the meaning of the terms "microeconomic foundations of macroeconomics" was stabilized and widely used only in the second half of the decade. They discuss collective volumes and surveys of the literature on disequilibrium, in particular Weintraub (1977), Malinvaud (1977) and Finally Drazen (1980), and the critical reactions they stirred up. They claim that the true content of disequilibrium theories was eclipsed by the success of Malinvaud. His presentation led to the assimilation of disequilibrium theories with fixed-price models that could easily be dismissed. The fact that the most sophisticated versions of these theories proposed a more general approach to the problem of how market worked was progressively forgotten. Backhouse and Boianovsky also mention the problem raised by the heterogeneity of these theories.

Chapter 9 intends to show that, in spite of the problems just mentioned, the disequilibrium line of research did not die out in the early 1980s: “the common perception that disequilibrium macroeconomics died at the end of the 1970s, killed by its failure to explain macroeconomic event is incorrect” (page 180). This basic claim is based on a broad review of research programs and works developed from the 1980s to the years 2000 including the joint works of Hahn and Solow (1995), Phelps, Stiglitz, Fisher, Diamond, Howitt, Rottemberg, Svensson, Bénassy, again, and, finally, Woodford. An ongoing source of inspiration until the 1990s, the disequilibrium approach would have survived in New Keynesian and New Neoclassical Synthesis models through their adoption of imperfect competition. For this reason, Backhouse and Boianovsky conclude that the “search for non-Walrasian microfoundations left a deep mark on macroeconomics” (page 186).

2. On the origins of disequilibrium theories

My comments will concentrate on my domain of expertise that is chapters 3 and 4 of the book concerning the relation between the works of Lange, Hicks, Patinkin and Clower. Although I appreciate many aspects of the presentation offered by the authors, it is marred by a number of inaccuracies.

Hicks and Lange

If chapter 3 rightly emphasizes the debt of Patinkin towards Hicks and Lange, the authors fail to bring out the nature of Patinkin’s innovation with respect to his mentors. Hicks (1939) and Lange (1938, 1945) introduced two ideas that led to the search for disequilibrium microfoundations. They claimed that the Keynesian theory was in need of more rigorous foundations and they suggested that such foundations had to be derived from Walrasian general equilibrium models. But they failed to see that accounting for involuntary unemployment required the assumption that production and exchange took place while market were not cleared and that such assumption challenged the Walrasian edifice.

Lange’s contributions illustrate my point.[2] Backhouse and Boianovsky rightly note that in “The Rate of Interest and the Optimum Propensity to Consume” (1938) he “compared Keynes with Walras” (page 33) and that he “used his diagrammatic apparatus to sort out historical

[2] For the case of Hicks see (Rubin, 2011 and 2014).

issues pertaining to the relation between Keynes's, Walras's and Malthus's respective ideas" (page 37). But much more was at stake. A careful reading of his paper shows that he used the reference to Pareto and Walras in order to make sense of Keynes' aggregate relations and to demonstrate the scope of his conclusions.[3] For him, Keynes's system was a simplified Walrasian model. This was important for Lange because it meant that Keynes had proven that even a perfectly competitive system could be subject to under-accumulation and underemployment. This assimilation was possible because Lange interpreted involuntary unemployment as a labor market clearing state with a horizontal labor supply curve.

In *Price Flexibility and Employment* (1945), a monograph strongly inspired by Hicks' *Value and Capital* (1939), Lange developed a different approach. In order to discuss rigorously Keynes chapter 19, he proposed to analyze the various parameters conditioning the stability of general equilibrium system with *n* commodities, money and bonds. For a certain constellation of parameters, the system would prove unstable. In more Keynesian language, once an excess supply had appeared on a factor market, the economy could never return to full employment. What economists had to do was to examine the actual situation of the economy and see if they could modify its parameters in order to stabilize the system. This approach was about disequilibrium since it focused on states of the economy in which supplies and demand differed. But the possibility and the implications of rationing were totally ignored. In Lange's perspective, a depression was a tâtonnement process lasting a decade. Though he did not discuss the issue, this implied that agents did not trade, produce or consume for years.

Patinkin

In Rubin (2012), I tried to show that Patinkin's PhD thesis (1947) foreshadowed the research program of disequilibrium theories in the 1970s. Patinkin's thesis was important because it introduced a concept that is totally ignored in the presentation of Backhouse and Boianovsky namely the concept of "additional restraint". Backhouse and Boianovsky note very briefly that Patinkin did not "accept Lange's claim that there was no essential difference between equilibrium and disequilibrium interpretation of involuntary unemployment" (page 38). This point should have been discussed more thoroughly for it marks the crucial move opening the whole field of disequilibrium economics as it came to be understood in the 1960s and the 1970s. Disequilibrium economics was about economic systems featuring rationing or agents that could not realize their notional or Walrasian plans. But how could one incorporate such individual disequilibria using the language of microeconomic theory? This was the key issue

[3] This detailed reading is provided in (Rubin, 2014).

ignored by Hicks (1939) and by Lange (1938, 1945). Patinkin was the first to raise it in his PhD thesis in 1947. Like Lange, he wanted to show how an *IS-LM* model could be derived from a Walrasian model. But he rejected Lange's definition of involuntary unemployment. Unemployment could not be involuntary if the labor market cleared. Workers had to be "off their labor supply curves". They could not obtain their desired level of employment but they had to accept instead a level of employment imposed by firms. The latter acted like an "additional restraint" in their optimizing plans. At his point, Patinkin realized that to derive a Keynesian framework from a Walrasian model, the latter had to be modified at two levels: the level of individual choices and the level of adjustment rules (what mechanism determined the nature and the magnitude of "additional restraints"). But if Patinkin captured the general insights leading to disequilibrium macroeconomics, he failed to give them an operational content and was probably disappointed by the results of his thesis. This explains how he discussed involuntary unemployment in chapter 13 of *Money, Interest and Prices*.

Backhouse and Boianovsky endorse the usual views of chapter 13 reading it as evidence that Patinkin did not "realize the full significance of what he was doing".[4] Had they paid sufficient attention to his PhD thesis, they would probably have seen this text in a different light. Patinkin was quite aware of what he was doing. But he was torn between his insights and the requirements of rigorous theorizing. In chapter 13, Patinkin vigorously pushed forward what he called "unemployment disequilibrium". He took up the insights of his thesis and even went further. He finally captured the operational intuition he had missed in 1947. Faced with a sales constraint in the market for goods, firms would have to revise their demand for labor without taking into account the level of the real wage. This so called "spillover effect" would become the key concept of the literature on disequilibrium in the 1970s. Backhouse and Boianovsky stress the absence of the terms "disequilibrium" and "non-market clearing" in the introduction of the book. But they were used time and again in Patinkin's key texts. As a matter of fact, he was the first economist to write that Keynesian theory was about disequilibrium (Patinkin, 1948: 563). And this conclusion was the main message of chapter 13 repeated on pages 315, 323, 327, 228 and on pages 337-8 of chapter 14, each time in the form of striking slogans. Did Patinkin realize that what was at stake was a "more general theory of how market worked" (page 41)? He surely believed that the issue was of a general nature. The words he used in his introduction to define the subject, quoted by Backhouse and Boianovsky themselves, were "the monetary theory of an economy with involuntary unemployment". But this theory

[4] See d'Autume (1985, page 104) or Bénassy (1984).

included full employment as a special case. This theory was the general theory towards which economists had to work their way, the "climax" of his book just like chapter 19 was the "climax" of the *General Theory* for Patinkin. Did he realize that this theory was something different from the Walrasian theory? Here is the main difficulty. I think that my work on Patinkin's thesis shows that he understood that his analysis implied behaviors incompatible with standard microeconomics. In 1947, he had attempted to devise an alternative micro. But his failure led him to a very conservative stance in later years. In 1956, Patinkin himself attracted the attention of his readers on the inconsistency between the behavior of firms in his Keynesian scenario and their Walrasian behavior. Disequilibrium transaction on the goods market "invalidated" the Walrasian plans (1956, page 216 and note 9 on page 220). But as he put it in a letter to Nissan Liviathan quoted by Boianovsky (2006: 234): "I am interested in working on the traditional acceptable way of price theory." Patinkin had burnt his fingers with fancy mathematics and was mindful of the dangers of embarking in ad hoc theorizing. He wanted to find a way to reconcile his analysis with standard microeconomics. But the task proved daunting and he could not formalize his theory. In this respect Patinkin had reached a deadlock.

Clower

Backhouse and Boianovsky rightly emphasize the importance of Clower's 1965 article on the "Keynesian Counter-Revolution". Clower played a crucial role because he broke Patinkin's deadlock. Just like the Patinkin of 1947, he was not afraid to modify choice theory deeply to account for involuntary unemployment. Furthermore, Clower had a stroke of genius. He realized what a whole generation of Keynesian economists had failed to realize. He saw that the Keynesian consumption function could not be derived from the standard consumer theory. In a Walrasian general equilibrium context, one could not assume that income was given like in introductory textbooks. A different theory was needed to explain how income could appear as an independent variable in worker's consumption function. His answer, the existence of trade out of equilibrium on the labor market was only the obverse side of Patinkin's spillover effect. But Clower managed to formulate the issue in general terms and, above all, he did what Patinkin had refused to do in 1956. He offered a mathematical transcription of his insight. Finally, he resorted to warlike rhetoric and proclaimed his heterodoxy in a way that could not fail to attract the attention of the younger generation at the end of the 1960s.

But what did his contribution owe to the works of Patinkin? Reading the presentation of Backhouse and Boianovsky, one is tempted to answer “nothing”. Clower would have reached his conclusion on the basis of an independent research program developed in 1950s. This interpretation can be questioned on the face of evidence.[5] Clower developed two lines of research during the 1950s. But in none of them he studied involuntary unemployment and in none of them he studied seriously the implications of disequilibrium transactions.

Clower’s first line of research attempted to give microfoundations to the Keynesian theory. In his PhD thesis, he dismissed the problem raised by Patinkin concerning the definition of involuntary unemployment (Clower, 1952: 66). A horizontal supply of labor curve and a labor market that cleared were enough to speak of fluctuations in employment, the only important subject. Like Patinkin, Clower wanted to reconstruct the theory of Keynes starting from the Walrasian general equilibrium theory. Unlike him though, he approached the matter from the perspective of the debates on loanable funds versus liquidity preference theory. What Keynes had introduced was a new theory of the rate of interest based on the role of stocks of bonds and money, a theory showing how speculation could generate instability. In order to incorporate this insight into the “traditional theory of prices”, Clower set out to “broaden” it. This led to the elaboration of stock-flow models in which all markets cleared. These models were supposed to show how the economy would fluctuate around a stationary equilibrium due to speculative behaviors. For various reasons, the research program petered out.

About Clower’s second research program, Backhouse and Boianovsky write that he “was searching for an alternative to the tâtonnement process” (page 49). It is correct to say that Clower considered situations of disequilibrium in which agents set prices and traded out of equilibrium in a series of papers between 1954 and 1959. The result of this research appeared in the conclusion of *Introduction to Mathematical Economics* (1957), a book ignored by Backhouse and Boianovsky. Clower’s aim was to see if the assumption of perfect competition was too restrictive. To answer this issue, he built a model describing the dynamics of a market in partial equilibrium. This model was supposed to allow for the unification of all forms of competitions from monopoly to perfect competition. But Clower’s partial equilibrium perspective led him to ignore completely spillover effects or, more generally, the consequences of disequilibrium trading on agent’s choices and on other markets. Ironically, he concluded with a plea for the use of competitive price theory as a tool “suitable for most

[5] What follows owes a lot to discussions with Romain Plassard. For a systematic argumentation of the following points see Plassard (2014).

theoretical and for nearly all practical purposes" (1957: 191). His disequilibrium theory of one market was already too complex to be of any use (1957: 190)!

So what happened to Clower? Well he experienced a decisive turnabout between 1960 and 1962. In 1960, Clower came back to the question of the relation between Keynes and the Classics. Like he had done during all the preceding decade, he maintained that both theories were perfectly compatible. But at the same time he built a Keynesian model in which consumers decided their level of consumption on the basis of a level of employment imposed by firms. In 1962, he had realized that this was not compatible with the standard theory of the consumer and claimed that Keynes and the Classics were totally incompatible. At this stage he focused on unemployment defined as an excess supply and portrayed Keynesian theory as disequilibrium economics. How can one explain such a change? It happens that, starting in 1958, Clower began to read *Money, Interest and Prices* and to interact intensively with Patinkin. In 1960, Clower invited him to Northwestern University where he stayed as a Visiting Professor. Actually, as I explained elsewhere (Rubin, 2005), the discussion between the two economists started about a paper (Patinkin, 1958) in which Patinkin examined the validity of Walras' law in a Keynesian context. This led him to write down a budget constraint in which, just like in Clower (1960a, 1965), the level of employment was imposed by firms. As a matter of fact, nearly all the ingredients of Clower contributions were already in Patinkin's writings. In 1965, for instance, Clower explained that Keynesian economics required the rejection of Walras' law. A careful reading of chapter 13 of *Money, Interest and Prices* shows that Walras' law is violated in Patinkin's dynamical analysis exactly like in Clower (1965), though Patinkin (1958, 1965) offered arguments to restore its validity. Other example, Backhouse and Boinovsky present a paper published by Clower in 1960 as the result of the work he realized in 1950s about disequilibrium dynamics. Actually, this paper was a simplified version of the conference on the relation between Keynes and the Classics that he gave at Northwestern University and the last sentences of the paper are nearly a paraphrase of Patinkin.[6]

In contrast with Backhouse and Boianovsky, I believe that Clower owed a lot to Patinkin's writings. Clower's importance came from the fact that he saw what Patinkin had not seen concerning the Keynesian theory of the consumer and from the way he challenged the Walrasian framework. The latter attitude may be due to his successive failures at "broadening" the traditional theory of prices during the 1950s. The idea that Keynesian

[6] Compare Clower (1960, page 323) to Patinkin (1956, page 220).

economics was about the dynamics of a flexible price system, a recurring slogan of Patinkin, was not original. The originality in Clower was his interest for instability which probably explains why he insisted on rejecting Walras' law.

On the micro/macro divide

Let me add a final remark concerning the origins of disequilibrium theories. I consider that Backhouse and Boianovsky exaggerate the distance between what they identify as the macro line of research of Patinkin, Clower or Barro and Grossman and the micro line of Arrow, Hahn and Negishi. All these economists shared the "search for disequilibrium microfoundations" that can be conceived retrospectively as an overall project written in the DNA of the Neoclassical Synthesis. The project sprang from a small set of seminal ideas. Firstly, Keynesian theory was in need of more rigorous foundations. Secondly, in order to elaborate these foundations, one had to start from a Walrasian general equilibrium model. Thirdly, the main issue raised by Keynes was the explanation of involuntary unemployment hence the Walrasian framework had to be modified in order to incorporate the possibility of individual disequilibrium. That implied a distinction between Walrasian plans and plans resulting from the existence of "additional constraints".

Backhouse and Boianovsky make an important contribution by stressing the importance of the group of economists around Arrow, Hahn and Nigishi in the history of disequilibrium economics. But if their mastery of the Arrow-Debreu version of the Walrasian framework distinguished them from Patinkin or Clower, these economists shared common intellectual references, mainly Hicks (1939), Lange (1945), Samuelson (1947) and Keynes (1936). Besides, they interacted in a numbers of ways. Arrow discussed the work of Patinkin when the latter was writing his PhD dissertation at the Cowles Commission. Hahn, who wrote a thesis under the supervision of Kaldor, began to discuss Patinkin's works on money in 1952. Negishi discussed with Clower around 1960 when the latter turned his attention to the relation between Keynes and the Classics (Negishi, 1960). Note also that Hahn and Negishi (1962, p. 465) referred to "spillover effects" a concept coming from Patinkin (1956). Finally, all these economists gathered in 1962 at the Royaumont conference where Clower presented his path-breaking article. I thus share the view put forward by Wade Hands in a recent paper:

> "What Keynesian and Walrasian economics evolved into –what they became- when they stabilized into textbook macro and (advanced) textbook micro during the 1950s and 1960s, was, at least in part,

> a result of the fact that they were joined together in, and co-evolved within the context of, the neoclassical synthesis" (Wade Hands, 2012, p. 120).

This means that in order to understand why and to what extent the different approaches to the original quest for disequilibrium microfoundations diverged, we need to explore and to circumscribe what all its actors had in common and how they interacted at the start.

3. On the fate of disequilibrium theories

My last comments will concentrate on the important chapters 8 and 9 of Backhouse and Boianovsky. These chapters raise important questions and put forward provocative claims. But I found the argumentation somewhat disappointing.

I begin with chapter 9. Backhouse and Boianovsky update an old idea (Solow, 1979a: 79). Disequilibrium economics was important because it inspired a large part of the works generally put under the New Keynesian label (and these works were partially incorporated into the New Neoclassical Synthesis). This claim calls for a very interesting research. What were the original intentions of the economists latter classified as New Keynesians: Stiglitz, Diamond or Mankiw? Did they consider their contributions as developments of disequilibrium macroeconomics? Or did they try to escape the limits of this approach? If they developed different classes of models, was it unwittingly or under the influence of the methodology promoted by Lucas? Unfortunately, the chapter offers an awkward and often unconvincing answer to these questions. For instance, Backhouse and Boianovsky try to demonstrate that economists were still interested in disequilibrium until the 1990s. They mention the works of Franklin Fisher (1983) or Neary and Stiglitz (1983) as evidence of this. But these pieces were among the last contributions of disequilibrium economics and had no offspring. They also quote from a number of works like Greenwald and Stiglitz (1987) or Mankiw and Romer (1991) which shows precisely that the various classes of models developed in the 1970s by disequilibrium theoreticians had been abandoned at the end of the 1980s. They present monopolistic competition, a central ingredient of current macroeconomics, as a remnant of disequilibrium macro. But the key reference here is a paper by Dixit and Stiglitz (1977) that seems totally unconnected to the disequilibrium literature. Finally, they present Diamond (1984) as a continuation of disequilibrium economics. But Diamond himself explained that his was an "equilibrium approach to macroeconomics" (1984: 46) and "an alternative to the Keynesian model" (1984: 44).

I think that one should recognize that the fixed-price models of the 1970s (whether micro or macro), the non-tâtonnement approach or Hahn's models of conjectural equilibrium were all left behind after the mid-eighties. Historians have to explain this. De Vroey (2004) argued that models of non-Walrasian equilibria emptied the concept of involuntary unemployment of its substance and, above all, that the rival paradigm of Lucas was more attractive. The book of Backhouse and Boianovsky, and in particular its chapter 8, suggests that there is more to be said on the subject. But their discussion of the debates surrounding disequilibrium models looks more like a preliminary survey than like a comprehensive analysis. The idea that Malinvaud's 1977 model eclipsed the more complex works is seducing but the argumentation is too short. What was the nature of the complexities characterizing the approaches of Grandmont, Hahn or Fisher? Did they face problems that they could not resolve? Then the story would not revolve around the success of Malinvaud but around the failures of Hahn and Fisher… Historical studies of the problems faced by attempts to analyze the dynamics of disequilibrium models, to incorporate imperfect competition or money would be valuable additions to the literature. Backhouse and Boianovsky mention external criticisms coming from Monetarists. But what was the weight of these attacks in the context of the 1970s? And they do not discuss thoroughly the possibility that disequilibrium economics faced a rival school that managed to change the rules of the game pushing the young economists of MIT and other institutions to leave en masse.

Conclusion

Backhouse and Boianovsky's book is a path breaking contribution opening many avenues of research. The price paid for the scope of the book is a lack of depth in the treatment of various questions, a problem that weakens the interesting conclusions of their study.

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